notably on the Severn, which, according to Mr. Alfred Tylor, F.G.S., is seen to best advantage with a rising sun from Stonebench Inn, about three miles below Gloucester.

1878.	Height above average.		1878.	Height above average.		1878.	Height above average	
	ft.	in.		ft.	in.		ft.	in.
Jan. 20 p.m.	0	4	April 17 a.m.	0	8	Sept. 1 a.m.	1	3
,, 21 a.m.	0	7	,, ,, p.m.	0	11	,, ,, p.m.	0	11
,, ,, p.m.	0	9	,, 18 a.m.	1	1	,, 2 a.m.	0	7
,, 22 a.m.	0	10	,, ,, p.m.	1	3			
,, ,, p.m.	0	11	,, 19 a.m.	1	3	,, 26 a.m.	0	1
,, 23 a.m.	0	10	,, ,, p.m.	1	0	,, ,, p.m.	0	7
,, ,, p.m.	0	7	,, 20 a.m.	0	9	,, 27 a.m.	1	0
,, 24 a.m.	0	4	,, ,, p.m.	0	5	,, ,, p.m.	1	4
			,, 21 a.m.	0	1	,, 28 a.m.	1	7
Feb. 18 a.m.	0	5				,, ,, p.m.	1	8
,, ,, p.m.	0	11	May 16 a.m.	0	1	,, 29 a.m.	1	7
,, 19 a.m.	1	4	,, ,, p.m.	0	2	,, ,, p.m.	1	4
,, ,, p.m.	1	5	,, 17 a.m	0	3	,, 30 a.m.	1	1
,, 20 a.m.	1	6	,, ,, p.m.	0	3	,, ,, p.m.	0	9
,, ,, p.m.	1	6	,, 18 a.m.	0	3	Oct. 1 a.m.	0	4
,, 21 a.m.	1	5	,, ,, p.m.	0	2			
,, ,, p.m.	1	3				,, 25 a.m.	0	2
,, 22 a.m.	0	10	July 31 p.m.	0	3	,, ,, p.m.	0	6
,, ,, p.m.	0	4	Aug. 1 a.m.	0	5	,, 26 a.m.	0	10
			,, ,, p.m.	0	7	,, ,, p.m.	1	0
Mar. 18 p.m.	0	3	,, 2 a.m.	0	9	,, 27 a.m.	1	2
,, 19 a.m.	0	9	,, ,, p.m.	0	9	,, ,, p.m.	1	4
,, ,, p.m.	1	3	,, 3 a.m.	0	9	,, 28 a.m.	1	2
,, 20 a.m.	1	6	,, ,, p.m.	0	6	,, ,, p.m.	0	11
,, ,, p.m.	1	9	,, 4 a.m.	0	3	,, 29 a.m.	0	8
,, 21 a.m.	1	9				,, ,, p.m.	0	4
,, ,, p.m.	1	7	,, 28 p.m.	0	1			
,, 22 a.m.	1	5	,, 29 a.m.	0	6	Nov. 24 a.m.	0	1
,, ,, p.m.	1	1	,, ,, p.m.	1	0	,, ,, p.m.	0	3
,, 23 a.m.	0	8	,, 30 a.m.	1	3	,, 25 a.m.	0	4
,, ,, p.m.	0	1	,, ,, p.m.	1	5	,, ,, p.m.	0	4
			,, 31 a.m.	1	5	,, 26 a.m	0	5
April 16 p.m.	0	4	,, ,, p.m.	1	5	,, ,, p.m.	0	3

From the above table it appears that the highest tides of the year will occur on March 20–21 and September 28. The heights will be found probably to exceed those of the Admiralty Tables, as I have employed larger factors in the necessary corrections to the semi-menstrual inequality.

As a London tide table appears to be a desideratum, I have been induced to publish one for next year, in which the "danger" tides will be distinguished in a new, bold, and unmistakable manner. EDWARD ROBERTS

3, Verulam Buildings, Gray's Inn, November 17

Rainfall in the Temperate Zone in Connection with the Sun-spot Cycle

THIS month's number of the *Nineteenth Century* contains an article on the connection of rainfall with the eleven years' cycle of sun-spots. It takes a carefully-selected area in which such a coincidence, if it existed, would be well marked. The great tract of water spreading southwards from Asia to the southern pole affords an arena for the undisturbed play of solar activity. It may readily be understood that any excess of solar energy has a more direct and uniform influence upon the rainfall gathered from this vast aqueous expanse, than it would have upon smaller areas of water intermingled with tracts of land, and cut off from each other by ranges of mountains, as in the European and American continents. Other reasons exist which would render solar influence a more directly potent factor in the rainfall gathered from the Indian Ocean than in that of the temperate zone. Without doing more than alluding to the fact that sun-spot activity is confined to a belt of considerable thickness on either side of the sun's equator, there are several well-ascertained causes which would render an excess of solar activity more directly felt in the equatorial regions of our earth than in those nearer the poles. While, therefore, I believe that the coincidence of a rain cycle and of a cycle of wind disturbances with the eleven years' cycle of sun-spots, has now been established as regards the Indian Ocean and the Madras rainfall, I am anxious to guard against the conclusion being pushed too far. The article in the *Nineteenth Century* proves much, but it would be a misfortune at this still early stage of the inquiry, if wider inductions were drawn from it than are justified by the evidence which it brings forward.

It seems right, therefore, to state that so far as my investigation of the rain returns of the temperate zone yet enables me to form an opinion, the cyclic coincidence of the rainfall with the eleven years' cycle of sun-spots, seems to shade off in extra-tropical regions until it ceases to exist at all. This opinion is based upon an examination of the returns of between one and two hundred stations in different parts of the world, but only with regard to one-third of them is the evidence sufficiently complete as to raise more than a presumption either for or against the existence of a cycle. Further, I have not yet been able, except in comparatively small groups of stations, to examine the monthly returns or to separate the winter from the summer rainfall. This separation forms one of the first essentials to arriving at a final opinion on the question. Subject to these remarks, I beg to state the facts with regard to the rainfall of the northern extra-tropical zone in India, Europe, and America. It is chiefly with the first and last-named countries that the present contribution will deal.

In my "Cycle of Drought and Famine," printed in India on the commencement of the late dearth, I mentioned that the rainfall which, in periods of minimum sun-spots, passes uncondensed over the Southern Presidency, might possibly "fall in the temperate zone. The excessive rain, if it takes place anywhere, will probably be found in India between the 22nd and 32nd degree of north latitude, to the south of the great Himalayan partition wall." The conjecture was based upon the configuration of the Indian continent, which, in its lower and middle regions, receives the rainfall gathered from a vast ocean, and is provided with a barrier at the upper end to arrest the rain-clouds on their further progress northward. Prof. Archibald's examination of the rainfall in Northern India now throws a clear light on this side of the question. He has published in the leading Calcutta paper, the *Englishman*, a series of carefully-compiled returns from stations within the ten degrees of latitude above mentioned. He shows that the rainfall of the sub-tropical region, from 22° to, say, 30°, is in some respects (but only in some respects) complementary to the rainfall of Southern India, and in a recent letter to me he thus summarises his conclusions:—First, the winter-rainfall of Northern India varies inversely with the sun-spots in a well-marked manner at all the stations. Second, the summer rainfall varies directly with the sun-spots, in a manner well marked in the north-western provinces, by no means marked in the lower provinces of Bengal, but sufficiently well marked when the returns of the several stations are combined.

Let us examine the meaning of these facts. The returns from Madras and Bombay (lately published in NATURE and elsewhere) prove that when the summer monsoon strikes Southern India, its aqueous burden varies directly with the sun-spots. Prof. Archibald's returns now show that the rainfall brought by the summer monsoon to Northern India also varies directly with the sun-spots. But they prove more than this. They show that the rain-clouds which, in years of minimum sun-spots pass over India without dropping their watery burden, are found, on their being stopped by the Himalayan partition wall, to be charged with a more than average surplus (so to speak) of moisture. In Northern India, therefore, the summer monsoon, on its passage up, brings, as in Southern India, a rainfall varying directly with sun-spot activity; but the winter rainfall, *i.e.*, the immediate rebound of the rain-clouds from the Himalayan barrier, varies inversely with sun-spot activity. I say the immediate rebound, for it must not be forgotten that the north-eastern monsoon (October to December), when it strikes Madras in its full development, after collecting its aqueous freight from the Bay of Bengal, follows the same law as the summer monsoon (May to September), and varies directly with the sun-spots.

Passing from the sub-tropical region of Northern India (22° to 32° lat.) to the temperate zone, we find the evidence of a cycle either very faint or altogether wanting. With regard to Europe, I am not yet prepared to offer any new facts. The existing evidence only amounts to this: (1) Mr. Baxendell, from observations for a comparatively short period but very carefully recorded and scrutinised, came to the conclusion that even at an English station, notwithstanding the manifold disturbing influences incident to our insular meteorology, changes take place in the rainfall as well as in the temperature and barometric pressure,

which correspond closely in their maxima and minima periods with those of sun-spots. (2) A more comprehensive survey of the European rainfall has so far failed to establish this correspondence. Dr. Jelinek's examination of fourteen stations, from 1833 to 1869, showed that the coincidence held good in fifty-two cases, but failed in forty-two. While frankly accepting this as evidence against a real coincidence, it should be remembered that a general law such as a common periodicity in sun-spot activity and terrestrial rainfall will be subjected to, and sometimes overruled by, the local surroundings of individual stations. (3) On the other side, Gustav Wex, from the recorded depths of the Elbe, Rhine, Oder, Danube, and Vistula, for six sun-spot cycles (1800–1867), found that the maximum amount of water occurred during periods of maximum sun-spots, while the minimum levels were reached in the periods of minimum sun-spots. The evidence, as regards Europe, is, therefore, conflicting; and it is safer for the present to reckon it as against a well-marked common periodicity. I hope at no distant date to submit the results of a new and more exhaustive examination of the European rain-registers.

I now proceed to the North American rainfall. Here, as in Europe, the question is complicated not merely by disturbing meteorological influences, such as the Gulf Stream, but by the uncertain value of the rain-returns. These are causes which even at a carefully supervised station render it difficult to estimate the number of inches yielded by long-continued or very violent snow-storms. At badly supervised stations, or in the case of private gauges where the supervision is apt to be of a still more hap-hazard character, these difficulties often suffice to render the returns quite worthless. Yet it is the latter class of records on which we have chiefly to depend in an attempt to deal with the American rainfall during a long series of years. Nowhere does meteorology now receive more careful and scientific study than in the Western Continent, but in many of the most valuable series the element of time is still necessarily wanting. The evidence hitherto received from America has, on the whole, been favourable to the existence of a common periodicity. Mr. Dawson, Geologist to the British North American Boundary Commission, found a correspondence, although by no means an absolute one, between the fluctuations of the great lakes and the sun-spot periods. This question has been lately revived and interpreted afresh by a distinguished meteorological observer in Northern India. Prof. Brocklesby's contributions to the *American Journal of Science* also point to a connection between variations in the sun-spot area and annual rainfall.

It was with a knowledge of these statements that I undertook a systematic inquiry into the American rain-returns. I ought at once to say that the result of that inquiry altogether fails to establish the existence of a common cycle, so far as concerns the temperate zone. I divided the American stations into four groups. The first group consisted of eleven stations in east coast or Atlantic States, lying between 40° and 45° N. latitude. The second group consisted of seven stations in Inland States, from 38° to 48°. The third group was intended to consist of stations in the West Coast or Pacific States, but I have obtained the returns (and those for a period altogether too brief) for only a single West Coast Station, San Francisco. I give them, however, for what they are worth. The fourth group consists of three coast-stations in the Southern States, between 30° and 33°; or just above the sub-tropical region with which Mr. Archibald's returns for the Bengal stations deal.

The results of the examination of the four American groups may be summarised thus: (1) Taken as a whole, the returns from the twenty-two stations do not exhibit any common periodicity between the rainfall and the sun-spots; nor do they disclose an eleven year's cycle corresponding to the one which I have shown to exist in the rainfall (at Madras and elsewhere) gathered from the Indian Ocean. (2) That as regards the three northern groups, stretching across the continent from 38° to 48° N. lat., the rainfall, so far as any symptoms of periodicity can be detected at all, tends to vary inversely with the sun-spots; but that it is impossible to discover any real periodicity whatever. (3) On the other hand, that as regards the southern group, between 30° and 33°, there are symptoms of a periodicity tending to coincide with the sun-spot variations; but that these symptoms are not sufficiently uniform in the small number of southern stations which I have examined, to justify any conclusion.

The calculations on which these results are based would occupy many pages, but their general line may be indicated in a few sentences. Thus the mean rainfall at the twenty-two stations during the years of *maximum* sun-spots for which the records have been obtained, was 37½ inches, while during the years of minimum sun-spots it was 39. The years of maximum sun-spots, together with the years immediately preceding, had a mean fall at the twenty-two stations of 40·2 inches; while the minimum years of sun-spots, taken together with the years immediately preceding, had an almost exactly equal rainfall of 40·1 inches. In the northernmost group of eleven Atlantic stations the mean rainfall of the years of maximum sun-spots was 39 inches, against an average of 41 inches in years of minimum sun-spots; in the second group of seven inland stations (38° to 48°) the mean rainfall of the years of maximum sun-spots was precisely equal to that of years of minimum sun-spots, being 33¾ inches in both; in the third group, San Francisco, the mean rainfall years of maximum sun-spots was 21 inches against 23½ inches in minimum years; in the fourth group of three southern stations (30° to 33°) the returns for the minimum and maximum years are broken; but taking these years and the preceding ones together, the mean rainfall of the years of maximum sun-spots with the years immediately preceding was 51 inches, against 48½ inches in the years of minimum sun-spots and immediately preceding ones.

The returns have also been examined by another method. I have shown elsewhere that the rainfall at Madras, and other stations around the Indian Ocean, follows a well-marked cycle of eleven years, with a maximum, minimum, and intermediate period, corresponding with the maximum, minimum, and intermediate period of sun-spots. The American stations not only fail to show such a correspondence, but as regards the three northern groups so far as any symptoms of periodicity exist, they point in the opposite direction. The fourth or southern group of stations, on the other hand, so far as they disclose a periodicity, tend to coincide with the periodical variations in the sun-spots. The following table will show this. The Madras rainfall in the tropics discloses a cycle closely corresponding with the eleven cycle of sun-spots; speaking generally, the American rainfall in the temperate zone discloses no such cycle; but the southern stations begin to furnish symptoms of such a cycle.

Table of Madras and American Rainfall Compared with the Eleven Years' Cycle of Sun-spots

Rainfall and sun-spots shown in the minimum, intermediate, and maximum groups of the eleven years' cycle.	*Minimum Group.* Mean of 11th, 1st, and 2nd years.	*Intermediate Group.* Mean of 3rd, 4th, 9th, and 10th years.	*Maximum Group.* Mean of 5th, 6th, 7th, and 8th years.	Remarks.
Eleven years' cycle of sun-spots (from Wolf's lists)	12·6	43·5	76·8	Common Periodicity well-marked.
	Inches.	Inches.	Inches.	
Eleven years' cycle of rainfall at Madras	40·3	49·0	53·5	
Eleven years' cycle of rainfall; mean of three stations around the Indian Ocean...	43·4	48·1	52·2	
North American Rainfall.				
Mean of eleven stations in East Coast States, 40° to 45° N. lat....	40·2	41·6	40·1	No common Periodicity.
Mean of seven stations in Inland States, 38° to 48° N. lat.	35·3	35·8	34·6	
San Francisco; West Coast Station, 38° N. lat.	22·9	19·9	22·3	
Mean of three stations in Southern States, 30° to 33° N. lat.	47·0	51·2	49·1	Symptoms of common Periodicity.

NOTE.—The sun-spot figures represent the relative numbers, reduced from Wolf. The rainfall is expressed in inches. The San Francisco returns deal with only twenty-one years, or not quite two complete cycles; much too short a period for any definite conclusion.

The records of the twenty-two American stations extend over brief periods compared with the Madras returns. Several of them disclose breaks or gaps; few of them have been kept with the minute care bestowed by the professional astronomical staff on the rain gauge at the Madras Observatory, and the value of most of the eighteen northern ones is rendered in some degree uncertain by snow-storms. It is probable, moreover, that better and much more complete returns are available to American meteorologists than I possess for the twenty-two stations which

I have examined. They will come to the criticism of my results with fuller materials than are available to me here, but so far as these materials enable me to form an opinion, the result is against the existence of a common periodicity in the sun-spots and in the American rainfall within the temperate zone.

Allanton, Lanarkshire, November 4 W. W. HUNTER

Contribution to the Sun-spot Theory of Rainfall

THE Lucknow Meteorological Observatory has been established since 1868, and regular observations have been recorded since that year under my superintendence.

In NATURE of December 12, 1872, Mr. Lockyer published a notice of Mr. Meldrum's discovery of the coincidence between the maximum and minimum sun-spot periods, and the maximum and minimum rainfall in certain places. After reading it I examined the annual rainfall at Lucknow from 1868 to 1872, and found that there was reason to believe that the rainfall at Lucknow followed the same cycle as that of the sun-spots. The figures were :—

1868	27·6 inches.
1869	41·9 ,,
1870	64·6 ,,
1871	65·0 ,,
1872	41·4 ,,

The equal amount of rainfall (41 inches) on both sides of the maximum fall of 1870 and 1871 was very striking, and as there was a rise in the rainfall from 1868 to 1870–71, and after that a decrease, and having just read Meldrum's discovery, I conjectured that the annual rainfall would continue to decrease till it reached its minimum. In my annual abstract, which I submitted to Government in April, 1873, and on the slender evidence of five year's rainfall, I ventured to state that if Meldrum's law be true, we had in Lucknow lately passed the period of maximum rainfall, and were descending towards a minimum, so that during 1877, 1878, and 1879 there would be a scarcity of rain, and in one of those years the minimum rainfall of the cycle would occur. I am now able to give the annual rainfall of almost a complete cycle, and the figures will speak for themselves :—

1867 was a sun-spot minimum period.

1868	27·6	Inches of rainfall in Lucknow.
1869	41·9	
1870	64·6	
1871	65·0	
1872	41·4	
1873	35·1	
1874	51·4	
1875	43·5	
1876	23·6	
1877	11·7	

(Up to date October 22).

This is October 22, 1877, and the total fall up to date has been only 11·7 inches, about a third of which fell in the months of January, February, and March. The fall during the rainy season of 1877 has been so small that great fear of a famine has been felt. I considered Meldrum's discovery so important that at the end of my annual abstract of meteorological observations for 1872, I inserted a long abstract of Mr. Lockyer's article in NATURE, in order to make the theory more widely known.

I believe meteorologists are on the track of a most important law. I would not expect the maximum and minimum rainfalls in every place to coincide with the sun-spot maximum and minimum so completely as that given above. Possibly in some places the figures might be reversed, owing to a changed direction in the water-bearing currents of the atmosphere; but that the changes occurring in the sun have a direct influence on rainfall there cannot, I think, be any doubt. E. BONAVIA

Lucknow, October 22

The Radiometer and its Lessons

I WISH that Prof. G. C. Foster had been more explicit in his answer to my letter; for as it is I cannot understand to what "variations of density" he refers. So far as I know there are no variations of density in the gas in question except those which arise from variations of temperature; but these variations of density certainly do not affect the rate at which heat diffuses into and through the gas, for this rate is independent of the density and for the same gas depend only on the absolute temperature and on the degradation of temperature in the direction in which the diffusion takes place. The variations of temperature do affect the rate of communication but only in proportion to the square root of the absolute temperature, and hence, in the case of the radiometer, only to an inappreciable extent.

It is obvious that the law of diffusion holds good only so long as the gas is undisturbed by convection currents. Such currents, which certainly exist, increase the rate at which heat is communicated to the gas, that is to say, the hot surface instead of being exposed to the action of still air is exposed to a wind which tends to increase the rate of cooling. But the velocity of the wind does not increase with the rarefaction, and the cooling effect of a wind of a certain velocity does increase with the density of the air. Hence, as I pointed out in my first paper, the motion of the air will favour the force resulting from the communication of heat less and less as the rarefaction is increased.

As regards Mr. Johnstone Stoney's theory. The post which brought me this week's NATURE brought me also a paper from Mr. Stoney, on which I venture to comment. In doing this, however, I may say that I have no wish to criticise what Mr. Stoney has written. The fact that Mr. Stoney has in no way referred to my work, although I preceded him by some two years, has relieved me from all obligation to discuss Mr. Stoney's theory; and I certainly should not do so now were it not that, as Prof. Foster has instanced this theory as disproving what I believe to be the truth, I feel bound either to show wherein it is wrong or acknowledge my inability to do so.

In the paper which I have just received,[1] Mr. Stoney starts with an assumption that, but for the effect of gravitation, "a flat stratum of gas in contact with a hot surface, A," and "everywhere subject to the same pressure" can exist in a state of equilibrium "except at the limits," without any passage of heat from the hotter to the colder part, although "within the stratum the temperature gradually decreases, from within outwards, from θ_1 the temperature of A to θ_2 the temperature of the surrounding gas."

In support of this assumption I cannot find that any proof is offered except that which is contained in the following portion of a sentence :—"We know, from familiar experiments, which show gases to be bad conductors of heat, that after the brief interval of adjustment a permanent state would ensue in which there would be no further change of density, or motion of heat, except by radiation."

Now this assumption and the statement in support of it—in both of which Mr. Stoney seems to have ignored the very existence of diffusion of heat in gases—are contrary to all experience as well as to the deductions from the kinetic theory of gases; for it follows directly from the kinetic theory, and has been abundantly established by experiment, that under no circumstances can there exist a variation in the temperature of a continuous layer of gas without heat diffusing from the hotter to the cooler part.

I think that I need say no more. This assumed condition of gas forms the base of all Mr. Stoney's reasoning, and although in a subsequent part of his paper he appears to me to have arrived at deductions which contradict his fundamental assumption, still this assumption may be held accountable for the anomalies which he has found. OSBORNE REYNOLDS

November 17

I BEG to call the attention of the readers of NATURE to the following passage at the commencement of Mr. Crookes's lecture at the Royal Institution on February 11, 1876, "On the Mechanical Action of Light" :—

"To generate motion has been found a characteristic common, with one exception, to all the phases of physical force." [Illustrations are then given of the production of motion by heat, magnetism, electricity, gravitation, sound, and chemical force.]

"But *light*, in some respects the highest of the powers of nature, *has not hitherto been found capable of direct conversion into motion;* and such an exception cannot but be regarded as a singular anomaly.

"This anomaly the researches which I am about to bring before you have now removed; and, like the other forms of force, *light is found to be capable of direct conversion into motion*, and of being most delicately and accurately measured by the amount of motion thus produced."

I cannot but suppose that Mr. Crookes and Prof. Carey Foster have alike forgotten the existence of this passage. If it does *not* convey an interpretation of the phenomena of the radiometer which is now admitted on all hands to be wrong, and imply a claim to the discovery of "a new mode of force," I am incapable of understanding the meaning of words.

I may add that one after another of my eminent scientific

[1] "On the Penetration of Heat across Layers of Gas," *Scientific Transactions* of the Royal Dublin Society, November, 1877.